Review of Schoene et al. 2020 for GChron:

Overview

This manuscript (Sch+2020) presents a new analysis that seeks to reconcile recently published 40Ar/39Ar (Sprain et al., 2019-S+2019) and U/Pb (Schoene et al., 2019-Sch+2019) geochronologic datasets for the Deccan Traps. To do this, the authors input the 40Ar/39Ar dataset from Sprain et al. (2019) into the Bayesian Markov Chain Monte Carlo algorithm (Keller, 2018) that was used to generate the U/Pb eruption model in Schoene et al. (2019). The 40Ar/39Ar age modeling in Sch+2020 results in a near constant eruption rate through the Deccan, requires no increase in eruption rate after the KPg boundary, and suggests that the KPg boundary based on the 40Ar/39Ar dataset spans a wide range of the available stratigraphy. The authors also show that the precision available on the 40Ar/39Ar dataset is unlikely to be high enough to resolve eruption pulses like those identified in the U/Pb dataset. The authors assert that this study (Sch+2020) was completed to correct claims that the eruption models presented in Sprain et al. (2019) and Schoene et al. (2019) do not agree. Overall, I'm happy to see this study and to see a correction of misinterpretations made regarding the conclusions of the two manuscripts. However, as written, this paper is part of the problem as it contains many misinterpretations of the Sprain et al. (2019) manuscript, and, in reality, the conclusions of Sprain et al. (2019) are supported by the results from this study (Sch+2020). I apologize in advance for the length of this review.

I'd like to start this review with some clarifications of Sprain et al. (2019). First, **Figure 4 from Sprain et al. (2019) was never intended to show or say anything about eruption rate**. Instead, this figure was made to highlight the clear discrepancy between climatic changes observed in the paleoclimate record vs. the eruptive volume of lava. Particularly that there is no major climatic change during the eruption of the Wai subgroup, which represents between 50-75% (depending on placement of KPg) of the volume of lava in the Western Ghats. This figure was intended to be illustrative only, and if I had known that it could be easily misinterpreted in this way, it would have been heavily modified. I admit fault with the poor choice of the term 'eruptive flux' in the figure caption. I see now how this misinterpretation occurred. The figure that was intended to show changes in eruption rate is our (Sprain et al.'s) Figure 2, where we plotted cumulative minimum volume vs age. This is the figure updated from an identical figure in Renne et al. (2015), and is similar to Figure 1 in Schoene et al. (2019), which were all used for the same purpose: to show changes in volume vs. time (as is done for the analysis in this new manuscript). In this figure, we presented our calculated eruption rates for pre-Wai (what we identified as most likely being pre-KPg), and for the Wai subgroup eruption, which are clearly printed on our Figure 2. In the text of Sprain et al. (2019), you'll clearly see that any discussion referring to eruption rate/flux references our Figure 2, and that Figure 4 is referenced only in our discussion of climate change. As such, the implication in this new manuscript (Sch+2020) that we intended our Figure 4 to comment on eruption rate is incorrect, and needs to be modified.

This leads me to my second point, **a significant increase in eruption rate at the KPg boundary due to the Chicxulub impact was not one of the major findings of Sprain et al. (2019)**. In our manuscript, the following is the only statement made regarding eruption rate:

"we determined a mean magma extrusion rate of 0.4 ± 0.1 km3/year, representing 124,000 km3 of lava, for units erupted before the KPB (comprising the Kalsubai and Lonavala

subgroups) and a mean extrusion rate of 0.6 ± 0.2 km3/year, representing 435,000 km3 of lava, for units emplaced after the KPB (comprising the Wai Subgroup) ***(Fig. 2).*** *These results suggest that the mean extrusion rate* ***may*** *have increased after the KPB."*

Within the uncertainty of our calculations, an increase in eruption rate is a possibility. However, we did not intend to imply that there is substantiated rate increase and this was not a significant conclusion of our paper. Unfortunately, the claim that we determined there was a definite increase in eruption rate at the KPg boundary has been propagated since the publication of Sprain et al. (2019), starting with Burgess (2019) who stated:

"Sprain et al.'s dates do not resolve high-flux eruption pulses, suggesting instead that most of the Deccan lava volume (~75%) erupted after the mass extinction and that the impact caused an increase in the overall eruption rate (11)."

I would appreciate if all references in the current manuscript (Sch+2020) that say our data "argue for an increase in eruption rate coincident with the Chicxulub impact" be modified to more accurately represent what was stated in our manuscript.

This leads to my next point: **We did not call for eruptive pulses nor did we attempt to calculate eruptive pulses from our data.** This is in large part due to our precision level and an understanding that we would not be able to resolve eruptive pulses, like those interpreted in Schoene et al. (2019), from our data. We did argue against the pulses specifically laid out in Chenet et al. (2007-2009), but this was to highlight the inaccuracies in their specific model (i.e. miss-assignment of the Latifwadi plateau to pre-C30n, and misplacement of the KPg boundary at the Ambenali/Mahabaleshwar boundary).

Instead, we state the following:

"When we combined our data with previously published high-precision dates (7), we found that the DT lavas erupted quasi-continuously for 991,000 years (see Fig. 2), from ~66.413 Ma ago [the date for Jawhar Formation (Fm.) sample KAS15-3] to ~65.422 Ma ago (the date for upper Mahabaleshwar Fm. sample PAN15-3)."

Therefore, the results from Sprain et al. are entirely consistent with those presented in this new study (Sch+2020), that the 40Ar/39Ar data support a near constant eruption rate through the Deccan.

Finally, a major point of this new study (Sch+2020) is that "the stratigraphic position of the Chicxulub impact within the 40Ar/39Ar dataset is much more uncertain than was presented within Sprain et al. (2019)". This statement confuses me a bit **because we were very transparent in our manuscript about the uncertainty in the position of the KPg boundary**. We clearly stated that within the uncertainty of our data, the KPg boundary could fall anywhere between the upper Lonavala subgroup, up through the Poladpur formation (as the authors acknowledge in this new manuscript). We did use an age-model to get a better handle on the age of the Bushe-Poladpur boundary to better assess the Richards et al. (2015) hypothesis. Note, the age presented in Fig. 3 is for the Bushe-Poladpur contact, not the KPg boundary. Below, is what is stated in our manuscript:

"The results of our age model indicate that the transition from the Bushe Fm. to the Poladpur Fm. at Ambenali Ghat occurred between 60,000 years before and 20,000 years after the

KPB. We cannot exclude the possibility that the KPB occurs within the Bushe or the lower half of the Poladpur Fm., but the most probable placement according to our model is ~25 m below the contact between the two."

I'm not sure how we could have been more transparent in presenting our results, as we clearly highlighted the caveats to the possible KPg boundary location in Sprain et al. (2019), even including alternate locations in our discussion of climatic effects. I do understand that the authors of this manuscript (Sch+2020) have a strong opinion on the model we used. However, it is interesting that the results from the Bayesian MCMC model in this study (Sch+2020) are actually more consistent with those from our Bacon model than what is presented by the authors. Both models suggest that the most probable location of the KPg boundary is between the Lonavala subgroup and the lower half of the Poladpur formation. Furthermore, both model results are in contrast to the U/Pb data, which assigns the top of the Poladpur as the most probable location of the KPg boundary. Ultimately, the new analysis still supports the conclusion from Sprain et al. that based on the 40Ar/39Ar data the most likely location of the KPg boundary is *near* the Bushe-Poladpur contact. Albeit, I admit "near" has a large uncertainty. This new manuscript (Sch+2020) should be modified to more accurately reflect what is stated in Sprain et al. (2019), and a discussion of the resulting probability distribution from the 40Ar/39Ar data, not just its spread, should be added.

Overall, it appears that this manuscript is a criticism to misinterpretations of Sprain et al. (2019) that have spread since the publication of the manuscripts and is not actually a criticism of what was stated in Sprain et al. (2019). However, looking at the studies that the Sch+2020 authors cite as reproducing our 'spurious' eruption model (Burgess, 2019; Henehan et al., 2019; Hull et al., 2020; Linzmeier et al., 2020; Milligan et al., 2019; Montanari and Coccioni, 2019), it's not clear to me that these studies did reproduce this 'spurious' eruption model (other than one figure in Linzmeier et al.).

As an example, Hull et al. (2020) states:

"In contrast, Renne et al. (13) and Sprain et al. (8) proposed that the vast majority of Deccan basalts were emplaced after the impact. Schoene et al. (7) largely agree with the basalt flow ages of Sprain et al. and Renne et al. (8, 13) but place the K/Pg boundary higher in the lava pile (i.e., in the upper part of, or above, the Poladpur Formation) and therefore propose major pulses of emplacement immediately before and immediately after the impact (7)."

*"Guided by published hypotheses for the timing and volume of trap emplacement, we tested five major Deccan Trap emission scenarios differing in the timing of volatile release: (i) case 1 (leading), with the majority (87%) of degassing taking place before the K/Pg boundary [after (10)]; (ii) case 2 (50:50), with half of the degassing occurring before and half after the K/Pg boundary [after the lower estimate in (8-**Sprain et al., 2019**)]; (iii) case 3 (punctuated), with four pulses including a major event just preceding the K/Pg boundary [after (7, **Schoene et al., 2019**)]; (iv) case 4 (lagging), with the majority (87%) of degassing taking place after the K/Pg boundary [inverse case 1 pre- and post-outgassing volumes (13)]; and (v) case 5 (spanning), with emissions released evenly throughout magnetochron C29r [after (12)] (Table 1)."*

To me, this representation of the Sprain et al. dataset is accurate. Our study does identify that the majority of the Deccan volume was emplaced after the impact, and in fact our lower estimate, which was used in the Hull et al. case 2, was estimated with the placement of the

KPg boundary at the top of the Poladpur formation, consistent with the U/Pb dataset. It is also important to note that Hull et al. is not directly modeling eruption rate, only timing of degassing, and that they did not use the lava volume estimates calculated by either Sprain et al. (2019) or Schoene et al. (2019).

Since the major focus of this new manuscript (Sch+2020) is to clear-up misconceptions, can the authors expand on what they think the misinterpretations/misconceptions are in each of the cited studies and if they think the misconceptions biased results?

Overall, most of our original findings in Sprain et al. (2019) are supported by this new study (Sch+2020) and as both groups initially stated in the press releases for our Science papers, our results agree significantly more than they disagree (although there is still an outstanding discrepancy in the location of the KPg). I'm happy for this present study to clarify the misinformation that is spreading regarding our studies, but feel it's very important that the manuscript be heavily modified so that original intent of our study/results are clarified in addition to the exact misinformation they seek to stop (citing specific examples from their cited references).

I have other line/section specific comments that I have included below. I additionally found an error in the calculated average precision level for the 40Ar/39Ar data and in Figure 4 (the plotted uncertainties are incorrect). These should be addressed before publication.

I'd like to end my review on the note that it is unlikely for either dating technique to capture the eruption history of LIPs alone. Both techniques have their advantages and disadvantages. The 40Ar/39Ar technique has the advantage that it can directly date the lava flows, but will always be limited by precision because mafic lava flows are not high in K. The U/Pb technique applied to zircon on the other hand, has the advantage of high-precision, but often cannot directly date the mafic lavas themselves and is reliant upon the availability of silicic materials. To get a full picture of the eruptive history and tempo of LIPs, we need to combine the techniques. But to do this, we first need to work as a community to improve intercalibration, as nicely summarized in this new manuscript, which should be the focus of future work.

Sincerely,
Courtney Sprain

General Comments:

-Ensure that you have received copyright permissions to reprint the figures from Science.

- Please be consistent on your units of age. You switch from ka to kyr throughout the manuscript when describing durations.

Figure 2: I think this figure should be deleted since the figure actually used to discuss eruption rate in Sprain et al. was our Figure 2, and your Figure 4 already encompasses these data.

Figure 3: I like this and will probably reuse a version of it in the future to avoid any confusion from our original Figure 4. However, in context of the confusion regarding Figure 4, I'm not sure this figure is necessary for this manuscript.

<u>Figure 4:</u> The placement of our data in the stratigraphic column appears slightly off from the originally published dataset (from our Figure 2). Additionally, some of the uncertainties appear to be slightly off, e.g. the third data point from the top should have a 2-sigma uncertainty of 130 kyr, but it appears to be plotting closer to 175 kyr. Please check your data placement and also check that the uncertainties are being plotted appropriately.

<u>Figure 5.</u> The uncertainties shown for our data in Figure 5 are incorrect. Either they include systematic uncertainty, or what was presented in Sprain et al.'s Fig. 3 was doubled, which you'll note in the figure caption stated that the uncertainties were already plotted at 2-sigma. First, this figure needs to be modified to show the analytic uncertainty (see Sprain et al.'s Fig. 1, section AMB for raw data). Second, I'm concerned as to what uncertainty was input into your Bayesian MCMC model. If it wasn't our analytic uncertainty, than this analysis needs to be redone. Also, we didn't "conclude that the KPB falls at the Bushe-Poladpur contact". What was stated in our manuscript is:

"We place the KPB horizon (dated at 66.052 ± 0.008/0.043 Ma via the 40Ar/39Ar technique on a volcanic ash located 1 cm above the Ir anomaly in eastern Montana, USA) (25) within or near the top of the Lonavala or the basal Wai Subgroup, roughly coincident with the observed transitions that are suggested to reflect a fundamental change in the DT magmatic plumbing system."

AND

"The results of our age model indicate that the transition from the Bushe Fm. to the Poladpur Fm. at Ambenali Ghat occurred between 60,000 years before and 20,000 years after the KPB. We cannot exclude the possibility that the KPB occurs within the Bushe or the lower half of the Poladpur Fm., but the most probable placement according to our model is ~25 m below the contact between the two. With these results, we cannot reject the hypothesis that the major transitions observed within the Deccan stratigraphy near the Bushe- Poladpur boundary are due to changes in the magmatic system caused by the seismic energy from the Chicxulub impact."

Please modify lines 670-672 to something like this:

"Note these results contrast somewhat from the conclusion in Sprain et al. (2019), who suggest that the KPB falls near the Bushe-Poladpur contact."

<u>Figure 6.</u> Please modify this figure to reflect that the average 40Ar/39Ar uncertainty is actually ~210 kyr, not 270 kyr. See comments below for clarification.

<u>Section 2:</u> This entire section seems unnecessary considering we did not use nor intend for Figure 4 from Sprain et al. to be used in discussions of eruption rate. We used our Figure 2, which calculates eruption **rate**, and is the same as the analysis here. Through context, it is clear in our manuscript that we do not use Figure 4 in our assessment of eruption rate. I admit that my use of the term 'eruptive flux' in the figure caption was poor and I would modify it if I could. But I don't think my poor word choice necessitates a whole section in this manuscript, especially when this figure wasn't used in our analysis.

<u>Section 3:</u> Please check that the correct error bars were used in your model for the 40Ar/39Ar data. They are incorrect in Fig. 5 and I don't know if this was just a plotting error, or if the wrong uncertainties were input into your model as well.

Section 5: I think an important piece to this section is missing and that is, if the FCs age from Kuiper et al. (2008) is correct, then it suggests that the U/Pb zircon ages for the KPg boundary and the Deccan are ~200 ka too old. It is technically possible for U/Pb zircon data to be 100's ka too old. This could be due to magmatic residence issues (which may not be fully corrected using the Keller et al. 2018 model) and additionally, since the zircon ages for the Deccan are collected from red boles (which are not technically ashes), it is possible they are reworked and represent maximum ages. Although, it may be unlikely that these effects are responsible for the ~200 ka difference, I think the possibility that the U/Pb ages are too old should be discussed in this section.

Line Edits:

Line 23-24: This sentence is a little misleading. The Renne et al. (2015) study has been superseded by Sprain et al. (2019). In Sprain et al. (2019), it was concluded that the Deccan erupted quasi-continuously, and that there may have been an increase in eruption rate after the KPg boundary (but the estimated eruption rate between pre-KPg and post-KPg lavas was not significantly different). Please modify this sentence to reflect the findings of Sprain et al. (2019). I suggest something like 'while the 40Ar/39Ar dataset was used to argue for quasi-continuous eruption that may have increased at the KPg boundary, coincident with the Chicxulub impact.'

Lines 28-30: This matches with the conclusions from Sprain et al. (2019).

Lines 30-32: This sentence is also misleading. In our paper we clearly stated that the KPg boundary could fall in the upper Lonavala subgroup, up through the Poladpur formation. We did not hide the uncertainty in the placement. We did use an age-model to get a better handle on its possible location, which resulted in the "most likely" location being somewhere near the Bushe-Poladpur boundary. The age provided by our Bacon model that is presented in the paper is for the Bushe-Poladpur boundary, and should not be interpreted as an age for the KPg boundary. I'm not sure how we could have been more transparent in presenting our results, as we clearly highlighted the caveats to the possible KPg boundary location in Sprain et al. (2019). Please modify this statement to reflect what is presented in Sprain et al. (2019).

Lines 33-36: This sentence should be heavily modified or deleted. It appears that a lot of this manuscript is a criticism to misinterpretations of Sprain et al. (2019) and is not actually a criticism of what was presented in Sprain et al. (2019). We provided caveats on the location of the KPg boundary (even including alternate locations in our discussion of the climatic impacts), we did not report a large increase in eruption rate after the Chicxulub impact (only stating that eruption rate "may" have increased), and did not intend for figure 4 to comment on eruption pulses. This message has been reiterated in every conference and public lecture on this material. I don't know how our interpretation or presentation of our data could have been clearer (other than modifying figure 4).

Line 62: Delete "at all".

Line 77: "presumed ash-bearing intervals" doesn't fully capture the nature of red boles. At best, they are weathering horizons. Although, zircons have been found from these horizons, there is still a possibility that they are reworked/detrital. I would modify this to be more transparent about what red boles might be.

Lines 83-84: This is incorrect. The figure that we used to calculate/show eruption rates is Figure 2 from Sprain et al. (2019), not Figure 4 (see comments above). I would eliminate your Figure 2 from discussion, as your figure that is actually using the correct figure from Sprain et al. is encompassed in your Figure 4.

Lines 82-84: I assure you, we did not intend to plot our data in a way to give the impression that there was an increase in the eruption rate associated with the Chicxulub impact. Again, in our text, it is clear that all discussions of eruption rate refer to figure 2, and that figure 4 is only referenced in the climate discussion. Please modify this statement. Currently, it gives the impression that your interpretation of our figure 4 was our intent, which I can 100% assure you was not, as supported by the text in Sprain et al.

Lines 90-93: I don't entirely agree that these studies are citing spurious claims from our papers. Can you be more specific, citing examples from each study? Also, technically, Hull et al. (2020) specifically argues against a large pulse of gas released right before the KPg. In regard to the position of KPg/pulses around the KPg, the 40Ar/39Ar and U/Pb datasets do differ, even using the new Bayesian analysis presented here (Sch+2020). Also, none of these papers appear to explicitly use the false "eruption model" from S+2019's Figure 4.

Lines 99-100: This again, is misleading as it implies that this interpretation of Figure 4 was our intent. I agree, the use of term "flux" was a poor choice and that this figure, out of context, could be misinterpreted. But the original intent of figure 4 could have been picked up by context in the text of our manuscript. I suggest rephrasing this sentence to say something like 'This confusion has arisen due to a misinterpretation of Fig. 4 in Sprain et al. (2019), which although uses the term 'flux', was never intended by the original authors to comment on eruption rate."

Lines 131-132: The figure showing eruption rate in Sprain et al. (2019) is our Figure 2, not Figure 4. Please correct.

Lines 138-140: I agree that I should not have called it flux and that this poor word choice, by myself, has led to unforeseen confusion. But again, I want to reiterate, that this was not the original intent of Figure 4, as noted in the text of our manuscript.

Line 145: Interesting to see the data replotted. I may use a version like this in all my future talks, to avoid the added confusion. But again, this figure was not intended to be used in the discussion of eruptive rate or flux. We used our Figure 2.

Lines 148-154: Yes, it makes sense to use our Figure 2, as this was the figure we also used to discuss eruption rate. In light of the new information about our figure 4, I think the section should be modified to reflect that Figure 2 from Sprain et al. (2019) was the intended figure that commented on eruption rate. Also, can you add more detail on your plotting strategy? Our Fig. 2 was meticulously put together using all available chemostrat logs, and additionally, unpublished logs from Steve Self and Anne Jay. As you know, creating a composite framework isn't easy! Did you take our logs, data thief our figure 2, or something else?

Lines 159-160: Please add "consistent with the findings of Sprain et al. (2019)."

Lines 162-163: Again, it would be nice to emphasize that Sprain et al. (2019) does not identify a definitive increase in eruption rate post-KPg. Our calculated eruption rates between the pre-Wai and post-Wai subgroup overlapped within uncertainty, and all that was stated in the text was that eruption rate "may have increased".

Lines 172-175: This sentence is a little misleading. The probability distribution for the U/Pb data also spans from the lower Ambenali to the Bushe, with the most probable placement near the top of the Poladpur. For clarity, I think this should be clearly stated in the text, worded similarly to what I've written above. Additionally, although the 40Ar/39Ar probability distribution is more disperse (assuming that the correct errors were used), it does appear there is a much higher probability that the boundary is at the base of the Poladpur, or within the Bushe or Khandala, than at the top of the Poladpur. This agrees with our original analysis in Sprain et al. (2019) that the KPg boundary, based on the 40Ar/39Ar data, is more likely *near* the Bushe/Poladpur boundary. So actually, your new analysis supports the findings of Sprain et al. (2019), and also highlights that the U/Pb dataset and the 40Ar/39Ar dataset still do not agree on KPg location. I think this needs to be clarified in the text. I suggest adding this to the end of the sentence in line 175: "…Poladpur Fm., with the most probable position for the KPg boundary falling between the Khandala and lower Poladpur Fms." This then more clearly follows to your next sentence.

Lines 196-199: Please check that correct uncertainties were used in your age model, as they are not correct in Figure 5.

Lines 199-201: Although this sentence is correct, it is omitting the fact that the resulting probability distribution still clearly shows that the most probably location for the KPg boundary based on the 40Ar/39Ar data is not at the top of the Poladpur, but is near the middle or base. As a reminder, this is entirely consistent with what was presented within Sprain et al. (2019), which stated:

"The results of our age model indicate that the transition from the Bushe Fm. to the Poladpur Fm. at Ambenali Ghat occurred between 60,000 years before and 20,000 years after the KPB. We cannot exclude the possibility that the KPB occurs within the Bushe or the lower half of the Poladpur Fm., but the most probable placement according to our model is ~25 m below the contact between the two."

I suggest revising this sentence to something like the following:

"..section, with the most probably position of the KPg boundary falling near the middle to base of the Poladpur Fm., similar to…"

I think this is a clearer summary of the results from your age model.

Lines 206-207: Again, this is generally consistent with our results. We only stated that there "may" be an increase in eruption rate, but this was based on calculating an eruption rate directly from our data and not modeling it, which overlapped within uncertainty.

Lines 209-212: How did you calculate an average precision of 270 ka for the 40Ar/39Ar data? I did it myself and get a value of ~210 ka, not 270 ka. Note, this calculation should be done on our analytic errors and also these data should be taken from Sprain et al.'s Figure 1. The supplemental table provided in Sprain et al. (2019) only included new analyses, not data from Renne et al. (2015), and did not show combined weighted mean ages for replicate analyses between the studies. These results are instead shown in Figure 1 of Sprain et al. (2019). Using these data, the average precision is 210 ka, not 270 ka. It's also important to note that the median uncertainty for our data is 168 ka, showing that a few lower precision dates are skewing the mean higher. I recommend using the median precision (which may also lower yours as well) not the mean, as the distribution of uncertainties cannot be assumed to be Gaussian. Regardless, our overall precision is better than what is stated here. This would also suggest a factor of 2-3 lower precision compared to the U-Pb dataset, not 4-5. Please correct.

Lines 235-237: Please modify this sentence in accordance with the correct 40Ar/39Ar dataset average (or preferably median) precision.

Lines 238-240: I agree that this is a powerful exercise. I will note that this is why we did not attempt to resolve pulses from our data in Sprain et al. (2019). So, overall, I agree with this assessment.

Line 245-250: I suggest that the authors include a little bit more discussion of what the time-scale of red bole formation may be (independent of geochronology) with some description of typical bole thickness, red bole pedogenesis, and geochemical time estimates (e.g. see Sheldon et al. 2003 and references thereof; for instance Dzombak et al. 2020 use these results to estimate that a 10 cm bole forms over ~ 10,000 years while a 60 cm bole may form over 100,000 yr - see their Supplement Table). Although I agree that there is significant uncertainty about the time-scale of red bole formation in general, in addition to their provenance, it would be very useful for a reader to have some context of what the different opinions are (to prevent a misinterpretation of results) as well as some relevant references. I strongly note that I am not suggesting this addition to argue against any interpretations of the U/Pb geochronology or the age model, but instead to give the readers a clearer context of the available observations regarding red boles (and their variety e.g. green boles etc) as well as how frequent they are stratigraphically. Please also include citations for line 246-247.

Sheldon, Nathan D. "Pedogenesis and geochemical alteration of the Picture Gorge subgroup, Columbia River basalt, Oregon." Geological Society of America Bulletin 115.11 (2003): 1377-1387.

Dzombak, R. M., et al. "Stable climate in India during Deccan volcanism suggests limited influence on K–Pg extinction." Gondwana Research (2020).

Line 258: Please change "(Renne et al., 1998)" to "Renne et al. (1998)".

Line 285: The parentheses are misplaced in this sentence. Please modify.

Line 325-326: It's a little misleading to say that our results push the limits of precision and accuracy for our method, as it implies that 40Ar/39Ar cannot resolve precision levels less than a few hundred thousand years. We are working by necessity with a non-ideal mineral for 40Ar/39Ar geochronology, which only has trace amounts of K. The fact that our precision is as good as it is, is due to running multiple replicate analyses. If instead, we were able to identify sanidine, or another K-rich mineral, within the red boles (or other silicic units in other LIPs), then our precision level would be more comparable to U/Pb. I suggest modifying this statement to say something like "…each method, noting that the 40Ar/39Ar method here was limited by dating a K-poor mineral."

Line 327-332: This sentence needs to be revised. We did not use our data in Sprain et al. (2019) in the way that has been suggested in this current manuscript to build a model of eruption rates for the Deccan Traps. This is clear in the text of our manuscript. It's also not clear that this "model" was reproduced in these other studies (other than in one figure in Linzmeier et al.). I'm happy for this present study to clarify the misinformation that is spreading regarding our studies, but feel it's very important that the original intent of our study be clearly stated. We did not intend for Figure 4 to be used in discussions of eruption rates (nor did we use it in that way), we did not call for a major 'pulse' of magmatism after the KPg and instead suggested that the Deccan erupted "quasi-continously", and we did not definitively call for a large increase in eruption rate after KPg (instead stating that it **may** have increased, but our eruption rates overlapped within uncertainty). This should be made clear in this manuscript. Additionally, the exact "misinformation" that the authors seek to stop needs to be clarified.

Lines 338-340: Please modify or delete this sentence as it is inaccurate. All that was stated in Sprain et al. (2019) is that eruption rate "may" have increased after the KPg boundary, but our estimates for eruption rate overlapped within uncertainty. This was not one of our "key" suggestions, as has been implied here.

Lines 356-359: I agree with the sentiment of this sentence, but as written it is implying that the way in which the data was presented in Sprain et al. (2019) was not up to "standard". This is unfounded as the main criticism of our work in this manuscript is based on a misinterpretation of one of our figures, not on the actual analysis as presented in the text of our manuscript. I believe we were very fair in our treatment of our data, and presented as many caveats as possible within a Science paper. If anyone were to read our manuscript again, they would come to the same conclusion. As such, I think this line should be deleted.

Line 682: Add a space between "2sigma".

Line 684: Delete "possibly"

Line 689: Add a space between "2sigma".

Line 695: Fix the parentheses used in this sentence.